

more definite results; as it is, I regret to have been obliged to tell an imperfect story.

I should like to conclude by acknowledging the great assistance given me in this work by Mr. Tyrer and by my students, Messrs. Gimmingham and Le Rossignol.

University College, April 4.

WILLIAM RAMSAY.

The Blondlot or n -Rays.

In this laboratory we have obtained uniformly negative results in experiments on the Blondlot rays. Our experiments were made with the help of seven observers, including five doctors, one student, and one laboratory attendant. Calcium sulphide screens rendered fluorescent in a separate room by burning magnesium were employed. They were brought into an absolutely dark room in which the observers had been kept for some time. Two forms of screens were used:—

(1) Flat screens on which a circular area on a slip of glass is covered by calcium sulphide.

(2) The later form in which a circular area at the back of a hemispherical lens is covered by calcium sulphide. The screens were made by Mr. Leslie Miller.

The screens were either held by the hands of the observers or were clipped on stands.

The observers were told first to look steadily at the screens and report any variation in brightness, calling out "bright," "dimmer," "dim," "brighter," &c., as the appearances seemed to change. Even with the screens on the slips of glass the observers after a few moments were able to call out the changes, *although there was no attempt at muscular contraction*. With the lens form of screen the changes in brightness were very marked.

We next attempted to find whether muscular contraction behind the screen caused an increase of brightness. Of course, where the observer sees a change in brightness without muscular contraction it is easy to be misled on this point. We made the observer continue to call out the degree of brightness, and we contracted the muscles of the arm behind the screens sometimes after he had called out "bright" and sometimes after he had called out "dim." In the great majority of cases the effect we looked for did not follow. In the few cases in which it occurred we naturally attributed the results to the changes in brightness which can be observed without any muscular contraction.

We next told our observers to look, as it were, into the distance beyond the bright spot, and to report on the brightness of the screens. When the accommodation of the eyes for near vision was relaxed they reported without exception that the brightness of the screens was constant, and that muscular contraction made no difference.

When observers were then asked to touch the backs of the screens, thus warming them, they reported an increase of brightness.

It is not easy to explain the phenomena we have described. We believe that there is difficulty in accommodating for the fluorescent circle, and that there is a wavering movement of the ciliary muscles, and probably also a wavering in the size of the pupils. Yet it is asserted that we can focus a point of light in a dark room, and it is difficult to see why the fluorescent screen cannot also be kept steadily in focus when it consists of a flat glass slip with fluorescent circle. In the case of the later, and presumably more successful form of apparatus, the difficulty is easily understood. In that form the fluorescent rays proceed from the back of a hemispherical lens, that is, from a point within the posterior principal focus, and they are widely divergent, and thus strain the accommodation of all but near-sighted people. The fact that in every instance we found that the light becomes steady after relaxation of the accommodation is very striking.

But the phenomena observed by us do not go any distance towards explaining the results described in M. Blondlot's papers. How is it that he and many of his compatriots see increase of brightness under conditions in which we see none? Is the explanation to be found in the paper by Heinrich, "Die Aufmerksamkeit und die Funktion der Sinnesorgane" (*Zeitschr. für Psychologie u. Physiol. d. Sinnesorg.*, vols. ix and xi.), in conjunction with our observations? Heinrich found after many careful experi-

ments that the pupil dilates when attention is directed to an object situated in the field of indirect vision, and that it dilates still more, during a short mental effort, such as a calculation. He found also that on directing attention to an object in the field of indirect vision the ciliary muscle relaxes, thus diminishing the curvature of the crystalline lens, and that during mental calculation this change is very marked, causing a curvature even less than that required for vision of a remote object. He found also that under the same conditions the axes of vision tend to become parallel or even divergent.

Can it be that the mental condition of some observers in a state of expectancy reacts on the intrinsic muscles of their eyes, and thus they see what they think they should see?

We have also experimented with the rays from a Nernst lamp, but without result.

JOHN G. MCKENDRICK.

WALTER COLQUHOUN.

Physiological Laboratory, The University, Glasgow,
March 29.

Learned Societies.

IN NATURE of March 10, Mr. Basset directs attention to the fact that referees frequently know less about the subject-matter of the papers than the author, and that their reports frequently contain errors from their not understanding the papers.

Had Mr. Basset held a brief for the opposite camp to that which he claims to represent, he could hardly have adduced more powerful arguments in favour of the referee system.

If a paper is of any value, the author must *ipso facto* know more about the subject-matter than anyone else. If he does not he is not the proper man to write the paper. But it is just because authors so frequently send up papers in a form in which other people cannot understand them that referees are necessary.

At present few people have time to wade through pages and pages of discursive and ill explained writings on the off chance that they may ultimately light on an interesting result. On the other hand, it is desirable that workers in one branch of science should have some insight into the general character of the investigations which are being pursued by specialists in other directions. Now I have before me a number of mathematical papers which contain no indication whatever of what the authors are driving at. They begin by putting down certain formulæ which the reader is assumed to know, and when they have twisted these formulæ about a bit they stop short abruptly without any obvious rhyme or reason. There are, of course, specialists who understand and appreciate these papers, but to the man who has specialised in applied mathematics or in a different branch of pure mathematics, the whole thing as presented appears meaningless. On the other hand, I have read with interest many well expounded papers dealing with such subjects as physiology, palæobotany, or psychology of the general character of which it is easy for anyone to form an estimate, even without previous university training. The functions of a referee should be to see that the arguments in a paper are clearly put forward, and that the main conclusions are prominently stated at the beginning or end in such a way that a general survey of the ground covered can be formed by the reader before the methods are examined in detail.

I believe that a useful purpose would be served if one of the referees of a paper were in each case selected on account of his *ignorance* of the subject-matter with which the paper dealt. Transactions would then be more readable and more widely read than they are at present.

It frequently happens, moreover, that results are buried in out-of-the-way corners of lengthy papers where they get overlooked, and that when someone has published similar results in a more accessible journal an undignified priority controversy is the result, whereas the original discoverer has only himself to blame for failing to present his subject in a readable form.

Learned societies are frequently penny wise and pound foolish in issuing their transactions *uncut*, consequently those who wish to study the contents have to waste much of their time in jaggings and often tearing the pages with a

paper knife. This inevitably means *so many more papers left unread.*

While on the subject of referees, I should like to protest against the impatience of many secretaries, who seem to expect the poor referee to neglect his university duties at a minute's notice and to give up his whole time to preparing a report for return post.

If Mr. Basset were to start a "British Journal of Mathematics and Physics" without adopting the referee system or some equivalent, what would he do when X. Y. Z. sent him a paper disproving the existence of gravitation, when L. M. N. wrote proving that the ether consisted of jerk-backs of energy, or when P. Q. R. called men of science fools for not agreeing with his view that the sun's photosphere was composed of diatoms of electricity?

All the same, a journal of the kind suggested, if published of a convenient size, and with the pages cut, would fill a distinct want which certainly exists. G. H. BRYAN.

Euclid's Definition of a Straight Line.

I HAVE long thought that by the words *εὐθεῖαν*, commonly translated *evenly*, Euclid means *symmetrically*. The symmetry can be tested by turning the line over; for instance, the edge of a flat ruler is straight if, when turned over, it coincides with its original position.

If a long rigid body is rotated, while two points, one in each end, retain their places, every line of particles joining the two fixed points describes a surface of revolution, which is symmetrical (in the sense intended) with respect to the two fixed points. The innermost of all such surfaces is of vanishing breadth, and is Euclid's straight line.

J. D. EVERETT.

11 Leopold Road, Ealing, March 29.

Spawning of the Plaice.

WITH reference to the letters of Prof. Herdman and Mr. W. Wallace as to the commencement of the spawning of the plaice this season, it may be stated that in the Moray Firth plaice were found spawning in the last week of December, and that spawning is not yet completed. The time mentioned is rather earlier than usual for this district, and it is not unlikely that spawning has been accelerated by the mild winter.

T. WEMYSS FULTON.

Aberdeen, March 29.

Fossil "Rain-drops."

THE preservation of impressions of rain-drops in slabs of Triassic marl has always presented some difficulty, since mud that was soft enough to receive such impressions would seem too soft to retain them.

I have to-day, at the borders of a flood plain, in a back-water of the Dorn Valley, near the Cherwell, seen exactly similar impressions in some stiff marly clay from which the flood water has lately subsided, where the surface of the tongues of clay is covered with the foot-prints of herons, rooks and smaller birds, with sun-cracks gradually widening until the clay curls into separate flakes, and the characteristic "rain-drop" pittings dispersed over the surface upon which no rain has fallen since the water subsided. This led me to seek another cause for these peculiar marks, and I soon found their origin. The film of mud over sand was in some cases still covered with about an inch of quiet water, and the decaying vegetation in the mud had given rise to innumerable bubbles that rested unbroken upon the mud bottom, like the bubbles adhering to the sides of a tumbler of soda-water. As the water very slowly leaves these bubbles some of them break, and some become coated (by surface tension, I suppose) with a thin film of mud which strengthens the dome, so that they can become larger, as they also become flatter, and sink slightly into the tenacious mud, which then contracts slightly away, so that the diameter of the circle is enlarged. When at length they disappear, they leave circular pits behind them in the half dried mud with a slightly raised ring edge, and finally, when the mud has completely dried, these shallow rounded pittings present exactly the appearance of Triassic "rain-drops" amongst the sun-cracks and foot-marks already alluded to.

I have not seen any such explanation of these "rain-drops," but it seems to remove a difficulty.

Oxford, March 19.

E. C. SPICER.

NO. 1797, VOL. 69]

THE USE OF LIGHT AND OTHER RADIATIONS IN THE TREATMENT OF DISEASE.

ONE of the most interesting fields of medical research at present is the investigation of the therapeutic properties of various rays, and although much has been accomplished in a few years, there is promise of a still greater future for this development of the healing art. Any advance in medical science is of the greatest moment to the general public, and cannot be too widely known, and in this respect this branch of therapeutics has had a measure of publicity which is probably unique, but which is not altogether free from harm. The discussion of purely medical details, and the description of "cures" of apparently hopeless cases in the columns of the lay Press, have unfortunately led to misconception and to terrible disappointment to many sufferers.

The fact that certain rays of light possess special physiological properties has been long known, and valuable papers on the subject were presented to the Royal Society as far back as 1872 by Downes and Blunt. But the credit of rendering the knowledge obtained by these and other observers of practical value in the treatment of disease belongs to Finsen, of Copenhagen. His first work was to show that the chemical rays of light, the violet and ultra-violet rays of the solar spectrum, have a deleterious influence upon the eruption of small-pox, and this led him to introduce the red light treatment for this disease. The patient is confined to an apartment from which the chemical rays are excluded by means of red curtains. For the treatment to be successful the curtains must be thick enough to exclude the chemical rays as completely as they are excluded by the photographer from his plates and films. In a patient under these conditions the ordinary course of the small-pox eruption is modified, the fever of the second stage is lessened, and the scarring is infinitesimal. It is not claimed that the mortality from this terrible disease is materially diminished by the light treatment, for in a certain proportion of cases there is no hope from the first, but in a large majority suffering is diminished, convalescence is easier, and disfigurement is slight.

Finsen's next work was the development of the light treatment for lupus. Lupus vulgaris is a very chronic destructive disease of the skin and mucous membranes caused by the bacillus of tubercle, the microbe which attacks and destroys the lungs in consumption. The chemical or actinic rays are here the therapeutic agents used. These rays have a definite germicidal power, and they are also capable of setting up a peculiar form of inflammation. They are the cause of sun-burns and of pigmentation of the skin from exposure to the sun's rays. In the treatment of lupus the rays of the sun, or, more conveniently in northern climates, those of a powerful electric arc light, are concentrated by means of lenses upon the diseased area. For the lenses rock-crystal must be used, because ordinary glass obstructs the passage of a considerable proportion of the rays in the ultra-violet part of the spectrum. When the sun's rays are used a light filter is employed to cut out as far as possible the heat rays at the red and yellow end of the spectrum. The light filter is a hollow lens filled with a solution of methylene blue or an ammoniated solution of the sulphate of copper. If the electric light is used the light filter is now dispensed with, as the proportion of heat rays is much less than in the rays of the sun. Even with the light filter a certain proportion of heat rays pass, and in using either the sun or the arc light it is found necessary to cool the surface under treatment. This is effected by placing in contact with the area treated an apparatus through which a current of cold water is constantly passing,